Acidic Proteins of the Nucleus. by Ivan L. Cameron; James R. Jeter, Jr.
Review by: James Bonner
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co-operation between cells in tissue culture, *P. S. G. Goldfarb, C. Slack, J. H. Subak-Sharpe and E. D. Wright;* Transport from symbiotic algae and symbiotic chloroplasts to host cells, *D. C. Smith;* Protein transport across the placenta, *A. E. Wild.*

This volume offers a wide-ranging view of transport processes, from movement of materials within cells, movement into and out of cells, to movement between cells. The topics covered are quite diverse; the organisms and cell types discussed range from foraminifera, amoebae, and fungi to higher plant sieve elements, transfer cells, mammalian neurons, and intestinal epithelium. Attention is given to structure as well as to function and physiology. Thus, the book offers the reader a valuable sampling of areas of investigation and, on occasion, conflicting points of views.

BARRY A. PALEVITZ, *Biological Sciences, State University of New York, Stony Brook*

ACIDIC PROTEINS OF THE NUCLEUS. *Cell Biology: A Series of Monographs.*

Edited by Ivan L. Cameron and James R. Jeter, Jr. Academic Press (a subsidiary of Harcourt Brace Jovanovish), New York, San Francisco, and London. $28.50. xvi + 343 p.; ill.; author and subject indexes. 1974.

This multiauthor volume is not the result of a symposium but is, rather, the result of a voluntary collaboration by several of the better known investigators in the field of nuclear and chromosomal proteins: Allfrey, Patel, Le Stourgeon, Totten and Foren, Wray, Kleinsmith, Magun, Berendes, and Helmsing, both of the editors, Spelsberg, and Gilmour.

Unluckily, the preface starts out by misspelling the basic word, "Protamine" as "Portamine." This mishap will undoubtedly augment the reader's annoyance with the volume's title. The nuclear and nonhistone chromosomal proteins are not properly termed acidic. All are less basic than histones, but they range from acidic to neutral to basic, and indeed sufficiently basic to be retained on Biorex P-70, as are histones. The volume could be more properly entitled, "Nonhistone Nuclear Proteins."

The currently fashionable suggestion that nonhistone nuclear proteins (and/or secondary modifications of such proteins by phosphorylation or acetylation) specify which DNA sequences are transcribed and which are not, may very well be correct, but it is thus far supported only by anecdotal evidence. Despite much effort there is still no agreement as to whether dissociated (by 2 M NaCl and 5 M urea) chromatin can be faithfully reconstituted, nor is there agreement as to the role of the nuclear nonhistone chromosomal proteins in such reconstitution. I shall return to this question below.

It has been known for several years that the major nonhistone chromosomal proteins (that is, those present in most abundant amounts) are very similar when the chromatin of different organs of similar creatures and indeed when the chromatin of different creatures is compared. This volume was written just as Le Stourgeon brought to realization the fact that the major nonhistone chromosomal proteins (half or more of the mass of this category) consists of proteins common to all tissues, such as RNA packaging proteins, alpha and beta tubulin, actin and myosin. The repeated efforts to find tissue specificity among the nonhistone chromosomal proteins has been made more difficult by the fact that these major proteins dilute those that may be of more interest. These facts are brought out most interestingly in the account by Le Stourgeon, et al. in the current volume. They suggest, as I have also suggested, that the presence of tubulin and actinomyosin proteins in chromatin may have to do with chromatin chromosome condensation at mitosis.

The confusion between major and minor, and possibly more, sequence-specific proteins continues through the volume. For example, in the chapter by Berendes and Helmsing, "Polytene Nuclei," the nonhistone chromosomal proteins which accumulate at puffing sites in about the 40,000 molecular weight region, may be merely RNA packaging proteins. They have not checked this, at least not in this volume. The same is true in the chapter by Jeter and Cameron. The chapter by Spelsburg, "Role of nuclear proteins in steroid binding," is a very complete and good review. It encompasses all of the things we have all learned about how steroids bind to a small (3.5S) cytoplasmic receptor protein, how this is modified in some unknown but reversible way to a 5S protein and how this then binds to something in chromatin. It probably binds to a chromosomal nonhistone chromosomal protein. The same is true for estrogen, progesterone, and probably for other steroids.

Finally, we come to the paper by Stuart Gilmour. He is concerned with the fidelity of chromatin reconstitution and cites the experiments of Spelsburg and Hnilca in 1973 as showing the importance of the nonhistone chromosomal proteins in reconstitution. The circumstances relating to this reconstitution have been described above. Not only is the nonhistone chromosomal protein not removed by their method of reassociation, but histone IV is also not removed. The dissociation of chromatin in NaCl and urea (5 M or thereabouts) by Gilmour and Paul, and reassociation by gradient dialysis cited by Gilmour in this review, are not totally understandable. The authors have shown that when brain chromatin is reconstituted with fetal liver nonhistone chromosomal protein, such chromatin produces globin transcripts which may be detected by complementary DNA to globin messenger. We now know, however, that when chromatin is dissociated, as has been done by them, and then reconstituted by gradient dialysis of NaCl and then of urea, the histones bind first and the nonhistone chromosomal proteins get no opportunity to put in their say as to where histones are to go until after the histones have already bound.

So, the volume is full of facts. It is interesting to read, but it is not full of things that one can accept as final truth. Let us hope that final truth will come in nearest future.

JAMES BONNER, *Biology, California Institute of Technology*

MOTILE MUSCLE AND CELL MODELS. *Studies in Soviet Science.*

By N. I. Arronet; translated from the Russian by Basil Haigh. Consultants Bureau, New York and London, $25.00. ix + 192 p.; ill.; no index. 1973.

This is a translation of Arronet's 1971 Russian account of the history of cell models and a review of their features and methods of preparation. Motile models are important because the elucidation of many essential problems is not accessible using homogeneous solvent-solute systems, in which the proteins have no structural continuity and cannot perform mechanical work, the essence of their function in vivo. Furthermore, the concentration of proteins, ions, and substrates in solution cannot be as high as it is in the microenvironments in structured systems, nor can the kinetics of protein-protein interactions in solution reflect the contribution of the specific spatial arrangements of structured systems.

Arronet emphasizes the full importance of the above issues by exemplification, describing many attributes of motile models that are neither predictable nor readily explainable by information obtained from homogeneous systems. The models discussed are those generated by disassembly: skinned muscle fibers, glycerol extracted muscle fibers, cells, ciliated epithelia, vorticella stems and trichocysts, myofibrils, thick and thin filaments; or by reassembly: reconstituted thick and thin filaments, actomyosin fibers. These models include systems that contract in ATP, elongate in ATP, oscillate in ATP, contract in calcium and elongate in calcium.

Models directly reveal differences between structures of apparently similar macromolecular composition. The microtubular elements of glycerol extracted cells respond differently to ATP. Chromosomal fibers contract, spindle fibers elongate. Glycerol extracted amebae contract in ATP, but glycerol extracted myxomycete plasmodia contract and relax cyclically.

Arronet discusses the use of models in studying problems of a non-mechanochemical nature as well. They have revealed the locus of the disfunction in poliomyelitus, and the site of sensitivity of muscle cells to insult by heat and pressure.

Arronet's rambling and repetitive style creates difficulties both in following his arguments and, since there is no index, in locating any particular item in the text. Nevertheless, and even though the reader is brought barely up to the state of affairs in 1971 by the text and references, all those working with motile systems will find this work a useful gadfly which can urge them to extend their thinking beyond the comfortably simplicity of homogeneous systems across the boundary to the more difficult but more interesting and relevant region of two-phase systems.

BERNARD D. TUNIK, *Biological Sciences, State University of New York, Stony Brook*

GENETICS AND EVOLUTION

HISTORY OF GENETICS. *From Prehistoric Times to the Rediscovery of Mendel's Laws.*

By Hans Stubbe; translated by T. R. W. Waters. The MIT Press, Cambridge, Massachusetts, and London, England. $14.95. xii + 356 p.; ill.; index. 1972.

This excellent translation has been made mainly from the second German edition, which was reviewed in *Q. R. B.*, 43: 85, 1968. Some revisions have been made, however, and an appendix has been added containing a translation of Mendel's letter to Nägeli of July 3, 1870. A supplementary English reading list has also been added. There is no other work to compare with this one in general comprehensiveness and accuracy for the period it covers; yet certain omissions may be noted, attention to which would make it even better. For example, although great attention has been given to the birth of cytology in the nineteenth century, as a harbinger of its eventual fusion with genetics, there is no account whatsoever of the early work on the nucleic acids, which Friedrich Miescher discovered in 1869 and which, after a lapse into neglect even longer than Mendel's work underwent, became the cornerstone of modern molecular genetics. Stubbe has also overlooked the remarkable work of Adanson in the eighteenth century, which utilized genetic breeding to demolish the cherished theory of Linnaeus about the origin of new species from hybridization and which instead proved the controversial cases of *Peloria* and other supposed new species to be, in modern terminology, mutants rather than new species (see Heredity and Variation in the Eighteenth Century, in *Forerunners of Darwin, 1745–1859*, B. Glass, O. Temkin, and W. L. Straus, Jr., eds.). In spite of a few such omissions, this fine, well-illustrated book will long remain an invaluable history of the early days of modern genetics.

BENTLEY GLASS, *Editor*

PRINCIPLES OF GENETICS. *Fifth Edition.*

By Eldon J. Gardner. John Wiley & Sons, New York, London, Sydney, and Toronto. $14.95. x + 622 p.